The Effect of Financial Audits on Governance Practices: Evidence from the Nonprofit Sector[*]

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Abstract

I evaluate the effect of financial statement audits on the governance practices of nonprofit organizations. Using a regression discontinuity design that exploits revenue-based exemption thresholds, I find that financial audits cause organizations to implement governance mechanisms such as conflict-of-interest policies, whistleblower policies, and formal approval of the CEO's compensation by a committee. Consistent with these governance practices curtailing managers' private benefits, I document reductions in nepotism and CEO-to-employee pay ratio. These results are more pronounced for organizations with an independent board of directors and with an audit committee, suggesting that audits lead to such governance reform primarily when the auditor reports directly to the governing body (as opposed to management). Collectively, my findings shed light on the role that financial audits play in shaping organizations' governance, beyond financial statements' direct use in decision making and contracting.

Keywords: auditing, governance, real effects, nonprofit organizations

JEL Codes: M42, G34, M48, L31

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1. Introduction

Each year, CPAs perform hundreds of thousands of financial statement audits for public companies, private firms, nonprofit organizations, and government entities. The broad literature that studies this economic activity naturally focuses on financial audits' primary role: to confer assurance over financial reporting quality (AICPA 2022a SAS 1, Defond and Zhang 2014). In this paper, I propose that financial audits also incidentally shape governance practices within organizations.[1] I empirically test this hypothesis in the nonprofit sector. Specifically, I evaluate whether financial audits cause charities to implement governance mechanisms, which can constrain managers' ability to extract private benefits.

In the U.S., public charities rely on financial support from the public, and are tax exempt under section 501(c)(3) of the Internal Revenue Code.[2] With approximately 300,000 organizations receiving over $1.7 trillion in annual income (McKeever 2015), public charities constitute an important sector of the economy. Furthermore, between 67% and 89% of U.S. households give to charities, and aggregate donations amount to 2% of the gross domestic product (Sullivan 2002, List 2011). Moreover, traditional monetary measures understate the economic importance of the charitable sector, because they do not capture the economic growth and productivity improvement that stem from addressing social issues.

In addition to its economic importance, the charitable setting is especially suitable for examining the effects of financial audits on governance practices. First, it offers extensive-margin variation in financial audits across organizations (i.e., certain charities obtain an audit, while others

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[1] Following Larcker and Tayan (2015), I define governance as the "collection of control mechanisms that an organization adopts to prevent or dissuade potentially self-interested managers from engaging in activities detrimental to the welfare of shareholders and stakeholders."

[2] Examples of public charities' missions include providing relief to the poor, the distressed, or the underprivileged, preventing cruelty to children or animals, and lessening the burdens of government (IRS 2021). Organizations in the healthcare and education sectors are exempt from the state-level regulations examined in this paper, as they fall under industry-specific audit mandates (Neely 2011, Desai and Yetman 2015).

do not).[3] Second, it features standardized governance data because charities must disclose whether they have specific governance mechanisms by answering yes-or-no questions in their annual IRS filings. Third, the charitable setting provides plausibly-exogenous variation in financial audits due to exemption thresholds in state-level audit mandates. Fourth, charities have agency conflicts between managers (who can extract private benefits) and stakeholders (who are represented by the board of directors) (Fama and Jensen 1983, Andreoni 2006, Core et al. 2006).[4] This choice of setting is also motivated by Leuz and Wysocki's call (2016) to use nontraditional settings to examine situations where the reporting entity changes its behavior in response to interventions like a financial audit. Although, results from the charitable sector may well apply to other important types of organizations (e.g., local governments and start-up firms with noncontrolling investors), I acknowledge they may not generalize to certain sectors of the economy (e.g., large public companies might maintain extensive governance mechanisms even if they were unaudited).

Drawing on the institutional features of the audit process, I predict that, although financial audits aim to confer assurance over financial reporting, they also incidentally shape governance mechanisms within organizations. During the audit, auditors assess their client's internal controls *over financial reporting* (AICPA 2022a AU 315). However, I argue that certain governance mechanisms can also act as such controls. For example, auditors may find a conflict-of-interest policy desirable because it helps them gain assurance over the completeness of related-party-transaction disclosures. A conflict-of-interest policy, however, also constrains managers' ability to engage in nepotistic behaviors. While nepotism typically translates into agency costs, it is of little concern to auditors because it has a limited effect on the reliability of financial reporting.

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[3] This paper specifically intends to draw inference on the consequences of obtaining a financial audit (i.e., as a bundle), relative to being unaudited.

[4] For instance, agency costs that stem from a principal-agent relationship between the board and the manager is less common in small private firms because the manager often turns out to be the owner.

In addition, auditors may evaluate governance practices that do not qualify, even indirectly, as internal controls over financial reporting if they believe that these practices foster a "culture of honesty and ethical behavior," which audit guidance links to decreased audit risk (AICPA 2022a AU 315). Perhaps more importantly, auditors often bundle their audit services with informal consulting.[5] This may include advice to the board of directions about governance mechanisms to consider implementing. Refer to section 2.1 for anecdotal evidence on auditors marketing themselves as informal advisors and emphasizing how their insider access helps them in this role.

Auditors can use their communication channel with "those charged with governance" as an opportunity to recommend specific governance measures (AICPA 2022 AU 380.25). The board may follow the auditor' advice because directors (1) learn about best practices in general or organization-specific areas where managers can act against stakeholders' interests, (2) believe it can reduce future audit fees (Bell et al. 2001, Lyon and Maher 2005, Hribar et al. 2014), or (3) can use the auditor's recommendation to support governance reform when directors and managers historically disagreed on the matter.

Despite the arguments above, the null hypothesis that a financial audit has no significant effect on governance mechanisms is plausible for several reasons. First, the purpose of a financial audit is to assure that financial statements conform to GAAP, not to reform governance practices (AICPA 2022a SAS 1). Second, auditors know that managers have significant control over their retention, and may fear that recommending governance measures that curtails management' private benefits will displease managers and cost them a recurring audit contract. Third, directors

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[5] Although the AICPA rules restrict auditors from providing paid consulting services that could impair their independence, they generally allow auditors to act as advisors, including in areas outside of the accounting function. For instance, auditors may "provide advice, research materials, and recommendations to assist management in performing its functions and making decisions; attend board meetings as a nonvoting advisor; [...and] provide management with advice regarding its potential plans, strategies, or relationships." (AICPA 2022b).

too may prefer not to push for governance measures because remaining on the good side of management helps them unlock the networking opportunities, prestige, and career advancement they often seek from a position on a board. Fourth, audit standards urge auditors not to expect "small, less complex organizations" (such as the charities I study in this paper) to adopt formal governance-related controls.[6] Fifth, auditors may not see the value in assessing and recommending governance changes because nonprofit audits carry little litigation risk.[7] After all, auditors and their nonprofit clients may view these audits—which are triggered by state regulations and not requested by contracting parties—as mere compliance exercises. Finally, directors may in fact relax governance practices and redirect their resources toward the charity's programs if they perceive a financial audit allows them to outsource their monitoring role.

Given these opposing theoretical arguments, understanding whether financial audits cause organizations to implement governance mechanisms is an empirical question. However, it is challenging to evaluate due to endogeneity concerns (Lennox et al. 2012). For instance, a recent fraud scandal in the nonprofit sector may prompt charities to both obtain an audit and strengthen their governance, resulting in a spurious association. Similarly, size is a potential confounding factor because audits and governance mechanisms, which both have fixed-cost components, simultaneously become more affordable as an organization grows.

To alleviate these concerns, I propose a regression discontinuity (RD) design that exploits regulatory thresholds that trigger the requirement to obtain a financial audit. These thresholds stem from state-level audit mandates, where each state enacts a single size threshold that exempts

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[6] For example, the standards say that "smaller entities might not have a written code of conduct but, instead, develop a culture that emphasizes the importance of integrity and ethical behavior through oral communication" (AICPA 2022a AU-C 315.A87).

[7] Private litigation and class action lawsuits against charities are uncommon because (1) small, dispersed donors have no formal contracts with the organization and (2) damage from mismanaged funds typically accrue to the beneficiaries instead of to the donors (Duguay 2022).

smaller charities within their jurisdiction. As of the end of my sample period, 23 states have such an audit mandate, and the exemption threshold ranges from $300,000 to $3,000,000 in revenue depending on the state.

The RD design mitigates endogeneity concerns by comparing above- and below-threshold charities that are similar in their other characteristics. The design restricts the estimation sample to a window around the threshold (i.e., the bandwidth), and flexibly controls for differences in size that could influence the propensity to implement governance mechanisms.

In support of the identifying assumption, I find no evidence of threshold gaming behavior (Lee 2008, Lee and Lemieux 2010, Breuer et al. 2018). I believe this is plausible because charities may have a limited ability to manipulate their revenue, which consists primarily of donations.

To identify governance mechanisms to use as the dependent variables, I inspect the IRS's Form 990 (i.e., the standard form charities must file annually) for yes-or-no questions about policies that prevent managers from engaging in activities that are detrimental to stakeholders (Larcker and Tayan 2015). I find that organizations are required to indicate whether they have a conflict-of-interest policy, a whistleblower policy, and formal approval of CEO compensation by a committee.

I document that undergoing a financial statement audit increases the probability that charities have a conflict-of-interest policy (by 20 percentage points), a whistleblower policy (by 23 percentage points), and formal approval of CEO compensation (by 19 percentage points). Next, I evaluate outcomes that proxy for managers' ability to extract private benefits. I focus on nepotism and the CEO-to-employee pay ratio because these proxies are less likely to change due to better compliance with GAAP (see discussion in section 4.2). For both proxies, I find a negative association with financial audits.

To ensure my results are robust, I perform a placebo test using states without audit mandates and where I assign placebo thresholds to charities (Roberts and Whited 2013). Comfortingly, I observe no discontinuity in the outcome variables around the placebo thresholds. I also test that my results are robust to alternative specifications.

Finally, I carry out additional analyses to learn about the mechanism by which financial audits shape these governance practices. First, I exploit the fact that review engagements do not involve internal-control testing (AICPA 2022a SSAE 22.07), but still offer assurance over financial reporting (although to a lesser degree than audits). I re-perform my main analyses using financial reviews as the treatment variable and find no association with my outcomes of interest, consistent with the internal-control assessment (which is part of financial audits, but not reviews) being a key part of the mechanism.[8] Second, I predict that, to the extent management has voting power and control over the board, charities are less likely to implement governance mechanisms. The result is consistent with my prediction, and suggests that the board of directors plays a significant role in driving the effect of audits on governance practices. Lastly, I provide evidence consistent with auditors being less likely to propose governance mechanisms when the decision to select and retain the auditor lies with management (as opposed to an audit committee), in line with auditors fearing to lose a recurring audit contract.

This paper should be of interest to researchers and market players. First, it can shed light on why managers sometimes appear unwilling to obtain a financial audit even if it might pay for itself through lower cost of debt, increased access to bank loans, eligibility for government assistance, and more donations (Minnis 2011, Kausar et al. 2016, Duguay 2022). Part of the answer may be that audits indeed prompt directors to implement governance mechanisms that curtail

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[8] To conduct this test, I exploit the fact that some of the states with audit mandates states also have mandates for financial reviews based on a lower threshold (see section 5.1 for details).

managers' ability to extract private benefits—managers knowing it seek to avoid an audit to retain their private benefits.

In addition, it can inform debates about audit mandates in various sectors of the economy (U.S. Senate 2005, Minnis and Shroff 2017, Breuer 2021, Nakhmurina 2021, Duguay 2022, Bourveau et al. 2022). Regulators often view audit mandates as a tool for monitoring organizations on behalf of taxpayers, donors, or dispersed investors. Naturally, the audit outcome that regulators tend to consider is the higher reliability of the financial information.[9] However, this empirical exercise can inform regulators about whether financial audits also prompt charities to implement governance mechanisms, which can facilitate internal monitoring by directors and thus support regulators' objective.

In terms of the literature, my paper first contributes to the stream of work on the interplay between audits and governance. Prior papers document the direct, first-order mechanism by which audits increase the reliability of financial information, which is an input for the board's decision making (Lennox 2005, Armstrong et al. 2010). For example, timely and comparable audited numbers help directors assess managers' performance and stewardship, reducing agency costs (Kothari et al. 2010). In other words, in this body of work, the reliability of the numbers in the financial statements is the audit outcome that facilitate governance. My study posits a new channel: audits also cause entities to implement mechanisms like conflict-of-interest policies, whistleblower policies, and formal approval of CEO compensation, which can incrementally curtail managers' ability to extract private benefits.

This research area also includes a strand of papers that treat governance as given, and

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[9] For example, in the nonprofit sector, regulators indicate that audited financial reports allow them to identify charities that spend little or nothing on charitable programs (see, e.g., Hawaii Senate 2008). These regulators are invested in knowing that charities use their funds for public good because donations are subsidized through tax deductions (this phenomenon is similar to governments' incentives to monitor corporate taxpayers; see, e.g., Hanlon et al. 2014).

evaluate its influence on the inputs of the audit process (Defond and Zhang 2014). For example, Cassell et al. (2012) show that firms with stronger governance are more likely to hire a Big N auditor. My paper differs in that it focuses on the relation that goes in the opposite direction: financial audits shape the governance of organizations.

Furthermore, my findings speak to the consequences of an audit per se, and are unlikely to be inferable from intensive-margin variation in auditing (e.g., choosing a Big N auditor).[10] For this very reason, recent work explores settings with both audited and unaudited organizations (Dedman et al. 2014). My findings thus add to the budding list of outcomes associated with obtaining an audit per se—those previously documented include capital-market outcomes (e.g., changes in the cost of debt, access to capital, and capital structure; Minnis 2011, Cassar 2011, Bernard et al. 2021) and "real effects" (e.g., firms' investment decisions; Lennox and Pittman 2011, Kausar et al. 2016, and Barrios et al. 2019).[11]

My study also relates to the literature on governance in general. Several papers study the effect of governance mechanisms on executive compensation, managers' propensity to enjoy the quiet life, firms' value, etc. (Core et al. 1999, Bertrand and Mullainathan 2003, Gompers et al. 2003, Chhaochharia and Grinstein 2009, Ferreira et al. 2011, Black and Kim 2012). I extend this work by documenting that audit-induced governance mechanisms come with a reduction in nepotism and CEO-to-employee pay ratio. The governance literature also calls for studies that account for the endogeneity and simultaneity of governance decisions (Schoar and Washington

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[10] The bundle that is an audit (i.e., having an outside professional come in, perform audit work and report to those charged with governance) likely shapes organizations differently than modifying a particular input to the audit process conditional on already undergoing an audit. In addition, organizations that face extensive-margin variation in audits are institutionally different from public firms (i.e., the latter are SEC regulated and their auditors are subject to high litigation risk and stringent regulatory oversight).

[11] "Real effects" refer to situations where financial reporting causes reporting entities to change their behavior in the real economy (Kanodia and Sapra 2016, Leuz and Wysocki 2016, Roychowdhury et al. 2019, Shroff 2020).

2011, Wintoki et al. 2012, Roberts and Whited 2013).[12] My paper answers this call by proposing threshold-based mandates in the nonprofit sector to help identify the causal effect of audits on governance.

Finally, I contribute to the nonprofit accounting literature, part of which examines questions from the audit and governance literatures specifically in the nonprofit setting. For example, prior work documents how nonprofit audits affect donations and reporting quality (Vermeer et al. 2006 and 2009, Kitching 2009, Garven et al. 2018, Duguay 2022), as well as how nonprofit governance mitigates agency problems, leading to more donations, a decrease in excess endowments, and fewer asset diversions or donor advisory warnings (Core et al. 2006, Yetman and Yetman 2012, Desai and Yetman 2015, Harris et al. 2015, Harris et al. 2017, Saxton and Neely 2019). Echoing my contribution to the audit and governance literatures, my paper adds to nonprofit research by proposing that nonprofit audits lead to the implementation of governance mechanisms, reducing nepotism and the CEO-to-employee pay ratio among charities.

2. Conceptual Underpinnings and Institutional Setting

2.1 Conceptual Underpinnings

Financial statement audits aim to confer assurance over financial reporting, yet they may also shape governance practices. One reason is that the generally accepted auditing standards (GAAS) instruct auditors to gain an understanding of the entity, its environment, and the risks of misstatement (AICPA 2022a AU 315). The guidance points to identifying internal controls *over financial reporting* (Lawrence et al. 2018). However, some of these internal controls may also constitute governance mechanisms, as they restrict management's ability to act against the interests

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[12] For instance, growth opportunities may prompt organizations to simultaneously obtain an audit and implement governance mechanisms in their attempt to raise external funding. Isolating the effect of audits on governance therefore calls for a research design that accounts for unobservable variables like growth opportunities.

of the stakeholders. For example, auditors may find a conflict-of-interest policy desirable because it helps them gain assurance over the completeness of related-party-transaction disclosures. At the same time, a conflict-of-interest policy constrains management's ability to engage in nepotism. While nepotism typically translates into agency costs, it is of little concern to auditors because it has a limited effect on the reliability of financial reporting.

Furthermore, auditors may even care about governance practices for which they see no direct link with financial reporting. Such governance mechanisms can collectively contribute to creating a "culture of honesty and ethical behavior" that, in conjunction with other controls, deters malfeasant managers and decreases audit risk (AICPA 2022a AU 315). In addition, auditors often cater to less sophisticated organizations by bundling their audit services with informal consulting.[13] As part of this informal advisory role, auditors may recommend governance mechanisms that do not necessarily pertain to the client's accounting function.

One natural question is why charity directors wait until their organization undergoes a financial audit to promote governance. After all, directors who seek to maximize stakeholder value should adopt governance measures as long as the ensuing reduction in agency costs outweighs the cost of implementation (Larcker and Tayan 2015). The answer is likely that an audit can affect the costs of implementation (or the directors' assessment of these costs) in several ways. First, the

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[13] A simple Google search of the keywords "CPA," "audit," and "nonprofit" reveal numerous examples. For instance, in marketing their audit services, audit firms write, "Our *advisors* develop insights through financial statement audits […] to help not-for-profits strengthen internal controls and *identify areas to improve operations*." (emphasis added, Anders CPAs 2022); "Audits, reviews, and compilations are a guilty pleasure with us. We get to know your business intimately so we can offer you an expert assessment with an insider's knowledge. Whether you need us to […] listen to your new business plan, we'll lend you a hand […]." (Perkins & Co 2022); "When you select Raffa-Marcum's Nonprofit & Social Sector to perform and manage your audit, you get seasoned professionals who provide *value beyond this assurance*. As an experienced audit firm and trusted partner and advisor […]" (emphasis added, Marcum 2022); and "When you work with LaPorte you'll have access to services that go *beyond compliance*. With a focus on best practices and solutions, our practitioners work as *proactive business advisors*." (emphasis added, LaPorte CPAs 2022). Another firm indicates its audit services come with "complimentary consultation hours and a library of free resources." (Gross Mendelsohn 2022). Finally, a guide on how to select an auditor recommends that nonprofit organizations ask prospective auditors about their "advisory capacity" and to "describe any other areas […] your approach can add value to our organization" (Wallace Foundation 2022).

board may learn about organization-specific areas where managers can act against stakeholders' interests. Indeed, outside board members are generally at an informational disadvantage because they are not involved in the day-to-day running of an organization. Through their insider access and professional expertise, auditors can identify issues the board is not aware of. Directors will then revise their beliefs about the costs and benefits of implementing specific governance mechanisms. Similarly, directors can learn from auditors about best governance practices. In fact, many nonprofit directors may have limited knowledge of governance due to a lack of business training or managerial experience (i.e., their expertise often pertains to social work, education, public health, etc.).

Second, directors and managers may disagree about the optimal level of governance. For example, directors may propose the implementation of tighter governance mechanisms, but management may disagree, arguing the suggested changes are not justified from a cost-benefit angle. Such a deadlock may be resolved in favor of the directors when the auditor recommends governance mechanisms.

Third, implementing governance mechanisms may reduce future audit fees because a stronger control environment can mitigate audit risk. A decrease in audit risk in turn allows auditors to lower their fees because they can restrict the scope of their work (e.g., by performing fewer substantive procedures; AICPA 2022a AU 312) or charge a smaller compensatory fee premium (Bell et al. 2001, Lyon and Maher 2005, Hribar et al. 2014, Duguay et al. 2020).

Despite these arguments, there are several reasons why a financial statement audit may have no significant effect on governance. First, auditors face strong financial incentives to gain the favor of management, as managers have considerable influence over auditors' appointments and retention (Duflo et al. 2013, Aobdia et al. 2020, Fung et al. 2021). Auditors also know that

managers will be less inclined to hire auditors with a reputation for recommending measures that keep management from extracting private benefits. Auditors can still address audit risk by suggesting controls that are unrelated to managers' private benefits or by raising audit fees (Hribar et al. 2014).

Second, board members also face conflicting incentives. Individuals often seek board positions for career advancement, networking opportunities, and prestige. These privileges are usually contingent on the CEO's favor (Bruynseels and Cardinaels 2014, He et al. 2017, Cassell et al. 2018, Bianchi et al. 2020). A board member who pushes governance mechanisms that constrain private-benefit extraction is likely to alienate the CEO. This director would directly bear the full cost of losing the CEO's favor, while the benefit of reducing agency costs would be shared among the entire pool of stakeholders (Zingales 2012).

Third, the auditing standards for evaluating a client's control environment contain considerations for smaller, less complex entities such as those studied in this paper. These considerations urge auditors not to expect smaller organizations to adopt formal governance-related controls. For example, the standards say that "smaller entities might not have a written code of conduct but, instead, develop a culture that emphasizes the importance of integrity and ethical behavior through oral communication" (AICPA 2022a AU-C 315.A87). It is therefore not obvious that auditors will recommend the implementation of formal governance mechanisms to the board of directors.

Fourth, auditors may see little value in assessing the governance practices of relatively small nonprofit clients where the litigation and reputational risk is limited (Minutti-Meza 2014, Chi et al. 2019, Lennox and Li 2020). Indeed, private litigation is uncommon among charities because they receive funding from small, dispersed donors with whom they have no formal

contracts (Duguay 2022). Class-action lawsuits against charities are also rare because donors almost never incur damage from misappropriation of donated funds (the beneficiaries do). Notably, the audits I study are triggered by state-level regulations rather than requests from contracting parties. Auditors are unlikely to recommend changes in governance practices if they, the managers, and the directors view the audits as a compliance exercise.

Finally, directors may view audits as a way to outsource their monitoring to a professional, and may thus relax governance practices to redirect time and resources toward the charity's programs. Such a reaction is especially likely if directors overestimate the scope of an audit, which can occur frequently in the nonprofit sector where decision makers often lack formal training in accounting or finance. [14] Thus, whether or not financial audits shape the governance of organizations remains an empirical question.

2.2 Institutional Setting

The organizations I study are public charities that receive financial support from the public and that enjoy tax-exempt status under section 501(c)(3) of the Internal Revenue Code. The purpose of these entities is described as "charitable, religious, educational, scientific, literary, [promoting] public safety, fostering national or international amateur sports competition, or preventing cruelty to children or animals" (IRS 2021).

Every year, public charities must file a Form 990 with the IRS, which then makes the form available to the public. The current version of Form 990 has existed since the 2008 tax year. In addition to reporting detailed balance-sheet and income-statement information, charities must also check boxes indicating whether or not they have specific policies in place. To identify the governance mechanisms to include in my tests, I inspect the Form 990 for questions related to

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[14] See the literature on auditors' role-perception gap (e.g., McEnroe and Martens 2001, Brazel et al. 2015, Tan and Yu 2018, Knechel et al. 2020, Bentley et al. 2021).

governance (as defined in this paper), and find that organizations must indicate whether they have a conflict-of-interest policy, a whistleblower policy, and formal approval of the CEO's compensation by a committee.[15]

States decide whether to impose mandatory financial statement audits on charities located in their jurisdiction as well as charities that solicit their constituents. These audit mandates always exempt smaller charities that fall under a revenue threshold. As of the end of my sample period, 23 states have a nonprofit audit mandate, and the state-specific exemption threshold ranges from $300,000 to $3,000,000 in revenue (see Appendix A). The healthcare and education sectors are generally excluded from these regulations because they are subject to industry-specific audit regulations (Neely 2011, Desai and Yetman 2015, Duguay 2022). Ten of the twenty-three states also have review-engagement mandates for charities whose revenue exceeds a lower revenue cutoff (see section 5.1 for details).

3. Research Design, Data and Sample

3.1 Research Design

To evaluate the effect of financial audits on charities' governance practices, I propose a research design that exploits revenue-based exemption thresholds.[16] As discussed in section 2, states with audit mandates always prescribe an exemption threshold for smaller organizations. This institutional feature allows me to use an RD design (specifically a fuzzy RD design because, for

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[15] The Form 990 includes disclosures about other policies that do not qualify as governance mechanisms (as defined in this paper). For example, charities must report whether they have a written document retention and destruction policy. Such a policy is generally not intended to prevent managers from extracting private benefits, but to ensure proper handling of donors' and employees' private information.
[16] Although this paper and Duguay (2022) both rely on the nonprofit audit setting, they propose different research designs because they study fundamentally different research questions. An RD design is appropriate for this study because (1) it measures organization-level outcomes and (2) allows me to exploit the threshold-based requirements as a source of variation in audits (i.e., the treatment is at the charity level and consists of an audit). In contrast, Duguay (2022) relies on a differential exposure design (1) to measure the allocation of donations among aggregate clusters of charities and (2) to derive an instrument for exposure to audit mandates (i.e., the treatment is at the aggregate level and consists of exposure to audit mandates).

instance, charities under the threshold can still voluntarily obtain an audit).

A fuzzy regression discontinuity design is equivalent to using an instrumental variable (Angrist and Pischke 2009). Thus, I express my main specification as a two-stage model:

$$Audit_{it} = \beta' Audit\ Required_{it} + \varphi' f(Distance\ to\ threshold_{it}) + \gamma' X + \varepsilon'_{it} \quad \text{1st Stage}$$
$$Y_{it} = \beta \widehat{Audit}_{it} + \varphi f(Distance\ to\ threshold_{it}) + \gamma X + \varepsilon_{it} \quad \text{2nd Stage}$$

$\left.\right\}$ Eq. (1)

Y_{it} is the dependent variable and varies across specification (e.g., Y_{it} represents governance practices in the main analysis). $Audit\ Required_{it}$ is the instrument, and is a binary variable equal to 1 when charity i exceeds the audit exemption threshold in tax year t. The treatment variable is $Audit_{it}$, a binary variable equal to 1 when a charity obtains an audit. An important statistical condition for this model is to have a strong positive association between $Audit\ Required_{it}$ and $Audit_{it}$ in the first stage (i.e., the relevance condition). $\widehat{Audit}_{it}$ is the coefficient of interest because it captures the local average treatment effect of an audit on Y_{it}. $f(Distance\ to\ threshold_{it})$ flexibly controls for the running variable, which is the log difference between the revenue and the threshold. The applicable threshold is the one prescribed by the state in which a charity is located, unless a charity registers in other states because it solicits out-of-state donations. In such a case, the applicable threshold is the lowest among the states in which the charity is registered or located. X is a vector of controls that includes *LnAssets* and *LnAge*.[17]

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[17] In Table A1, I add state and year fixed effects and show that the results are robust across all fives outcomes presented in the paper. I include the fixed effects in a robustness test rather than the base model because fixed effects impose stringent functional form assumptions to the RD model (which can impair the estimation of the relation between the running variable and the outcomes) (Lee and Lemieux 2010). Importantly, the RD method is distinct from other strategies based on treatment-control comparisons (e.g., difference-in-differences) in that its goal is not to use fixed effects and control variables to maximize treatment-control overlap, but rather to extrapolate the relation between the running variable and the outcome (Angrist and Pischke 2009). In other words, the specification with fixed effects helps support the validity of the RD design as opposed to improving it (Lee and Lemieux 2010).

To estimate the model, I use the Calonico et al. (2014) method because it presents two advantages. First, it reduces researcher discretion, while optimizing the RD parameters. Indeed, the method relies on a data-driven approach to identify the most efficient bandwidth and the best functional form for the running variable given the bias-variance tradeoff (Imbens and Kalyanaraman 2012). Second, it fosters conservative inferences because it produces bias-corrected estimates and relies on a robust variance estimator.[18]

I cluster standard errors by charity to account for across-time correlation within charities. Note that my RD design does not require clustering by state, unlike a difference-in-differences (DiD) design that exploits time-series variation in states' decision to adopt an audit mandate. Such a DiD design would raise concerns over across-charity dependence within each state because (1) the decision to pass an audit mandate is taken at the state level and (2) the design compares charities in treated states with those in untreated (or yet to be treated) states. In contrast, the RD design relies on cross-sectional variation by comparing charities around the revenue threshold. Furthermore, because only 23 states have audit mandates, clustering by state would result in too few clusters, potentially biasing my inferences (Conley et al. 2018). I also evaluate the sensitivity of my results to clustering standard errors by state $\times$ activity type (untabulated).[19] All inferences remain the same except for the effect on nepotism, which loses statistical significance.

One important assumption in this research design is the absence of omitted variables that correlate with the exemption threshold and affect the outcomes of interest. For example, if exceeding the threshold triggered not only the audit requirement, but also the obligation to, say, provide a social-impact report, then I could not attribute the observed effect solely to financial

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[18] Refer to Calonico et al. (2014) for a technical discussion of why conventional RD estimates fail to account for the bias that can result from the poor quality of the distributional approximations of the estimator.
[19] I define activity types using the two-digit codes from the National Taxonomy of Exempt Entities (NTEE).

audits. To address this concern, I carefully read the law to search for regulatory provisions other than the audit requirement that might become applicable upon exceeding the threshold. I find no such provision.

3.2 Test for Threshold Gaming Behavior

Having described the research design, I now evaluate whether charities manipulate their revenue to avoid a mandatory audit. In other words, I look for "bunching" under the threshold. My inferences would be threatened if there was bunching under the threshold and the charities that engaged in this avoidance behavior were more likely to have poor governance (e.g., because they had something hide). Under such a scenario, the difference in governance between above- and below-threshold would not be attributable to audits (Lee 2008, Lee and Lemieux 2010, Breuer et al. 2018).

I use two approaches to test for manipulation. First, in Figure 1A, I present a simple histogram of the density of observations around the audit thresholds, centering the state-specific thresholds at zero (e.g., Angrist and Pischke 2009, Breuer et al. 2018, Zimmerman 2019, Duguay et al. 2022). I find no visual evidence of an abnormal mass to the left of the threshold. Second, I perform a formal statistical test using the local density estimator method from Cattaneo et al. (2020). The test fails to reject the null hypothesis of no discontinuity (p-value of 0.677). Figure 1B illustrates the test by plotting the local density estimates with confidence intervals. The substantial overlap between the confidence bandwidths on both sides of the threshold suggests the absence of bunching.

The lack of threshold gaming behavior is plausible due to charities' limited ability to manipulate their revenue, which mainly consists of donations. In terms of real revenue management, charities cannot time the delivery of goods, unlike for-profit firms. Charities could

time their fundraising activities, but such a strategy would be imprecise because donated amounts are uncertain ex ante. In terms of accounting manipulation, revenue recognition for donations leaves little room for discretion in comparison to revenue from complex commercial activities.

3.3 Data and Sample

In this section, I describe my data and sample. I hand collect data on state-level audit mandates by using LexisNexis to read through relevant laws. I manually identify enactment dates, amendment dates, and exemption thresholds for all audit mandates imposed by state regulators during my sample period (see Appendix A).

I obtain Form 990 data from Guidestar for all public charities that file a Form 990 in tax years 2008 to 2015 and that are located or registered in states with an audit mandate. I drop observations with missing or impossible data as well as organizations in health and education (because these are subject to sector-specific audit regulations). This leaves me with 712,969 charity-year observations representing 134,626 unique charities. I report summary statistics in Table 1. The median charity-year observation has $519,000 in revenue and $748,000 in assets, 55% of observation obtain a financial audit, and 66%, 47%, 12% of observations have a conflict-of-interest policy, a whistleblower policy, and formal approval of CEO pay, respectively.

4. Main Results and Robustness Tests

4.1 The Effect of Financial Audits on Governance Practices

I begin by evaluating the effect of financial audits on the likelihood of implementing a conflict-of-interest policy. In Table 2, column 1, I report the first stage of the model from Eq. (1). I find a positive and strongly significant coefficient, suggesting that the audit mandates are enforced and that the relevance assumption is satisfied (Bernard et al. 2018, Bernard et al. 2021).[20]

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[20] The F-statistic for the first stage based on Sanderson and Windmeijer (2016) is above 50, providing additional support that the instrument is not weak.

Column 2 shows a positive and statistically significant estimate for the second stage, consistent with financial audits raising the propensity of charities to have a conflict-of-interest policy by 20 percentage points. In Figure 2A, I provide graphical support for the RD analysis by showing a binned scatter plot with fitted trends. In line with the tabulated analysis, I observe a jump in the likelihood of a conflict-of-interest policy around the audit threshold (Imbens and Lemieux 2020).

In columns 3-4 (5-6) of Table 2, I estimate Eq. (1) for *Whistleblower Policy* (*CEO Pay Approval*).[21] I find positive and significant coefficients in all four columns. In terms of magnitude, financial audits increase the probability that charities implement a whistleblower policy and formal approval for CEO compensation by 23 and 19 percentage points, respectively. Figures 2B and 2C provide a graphical representation of the RD design for *Whistleblower Policy* and *CEO Pay Approval*, respectively. Collectively, the results are consistent with financial audits causing charities to implement governance mechanisms.

4.2 The Effect of Financial Audits on Managers' Private Benefits

Next, I seek to evaluate the consequence of adopting these new governance mechanisms on managers' private benefits. This analysis poses two empirical challenges. First, certain proxies that capture private benefits can change due to better compliance with GAAP (i.e., audits can lead to changes in reported numbers even if real economic behavior remains the same). For instance, a common proxy for private benefits in the nonprofit setting is the ratio of expenses that go to charitable programs. This ratio suffers from this concern because the audits can force charities to change how they classify their expenses to conform with GAAP (Yetman and Yetman 2012, Burks 2015, Duguay 2022). Second, the increase in reliability of the financial statements could also lead to changes in real economic behavior. Using the same example, charities may view their ratio of

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[21] The number of effective observations changes slightly because the method identifies the most efficient bandwidth for each dependent variable.

program expenses as more comparable due to the audit. They may benchmark their ratio with peers and decide to make operational changes to either increase or lower it. The effect captured by the ratio would still be attributable to the audit, but not to the newly implemented governance mechanisms.

I propose two proxies that are less likely to suffer from these problems, although I acknowledge that I cannot complete rule out the concern. My first measure is the ratio of CEO to employee compensation: high CEO compensation can reflect rent extraction that stems from agency frictions. This measure is unlikely to change due to better compliance with GAAP because charities are required to report compensation numbers that match their W-2 forms.[22] In addition, in the nonprofit sector, compensation is much less commonly based on accounting numbers—for instance, the majority of charities do not have incentive compensation for their CEO (Balsam and Harris 2018). I proxy for CEO compensation by identifying the total compensation of the highest paid employee as reported on the Form 990. I measure average employee compensation as a charity's total compensation expense divided by its total number of employees. I compute the ratio by dividing CEO compensation by the compensation of the average employee. Deflating by the compensation of the average employee adjusts for labor market factors at the organization level. I then convert the measure to deciles for ease of interpretation and to ensure that extreme observations do not drive the results. I acknowledge limitations in terms of construct validity. High CEO compensation may be justified by competence or market conditions. Therefore, a decrease in CEO compensation cannot always be interpreted as a reduction in private-benefit extraction. Similarly, the number of employees may vary for reasons such as volunteer support. However,

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[22] Nevertheless, I acknowledge I cannot rule out this concern. A plausible scenario is the recognition of non-cash benefits as compensation. However, such a change would likely *increase* the CEO's reported compensation, which works against my finding a result.

such measurement error would likely attenuate rather than confound the results because it is unlikely to vary discontinuously around the threshold.

My second measure is nepotism, which I capture using family relationships. On the Form 990, charities must list the names of all their officers, key employees, and the top five compensated employees with a salary above $100,000. I define *Nepotism* as a binary variable equal to 1 if distinct key employees, officers, and board members share a last name. I recognize this proxy is imperfect, given that people can share last names without being family and that family members may have different last names (e.g., individuals who enter a marital union often take their spouse's last name). However, such measurement error would result in attenuation bias, which would work against my finding a result.

In Table 3, I present empirical evidence on the effect of financial audits on the CEO-to-employee pay ratio and on nepotism. Note that the sample size decreases because these specifications are based on the subsets of observations that have data on CEO compensation or on the names of key employees and board members. Columns 1 and 3 show that the first stage is positive and strongly significant for both outcomes, supporting the relevance condition. Column 2 shows that financial audits are associated with a 1.4-decile decrease in the ratio of CEO to employee compensation. Column 4 shows a negative and weakly significant relation between $\widehat{Audit}$ and nepotism. Figure 3 provides visual support for this analysis. Together, the results from Table 3 are consistent with audits constraining managers' ability to extract private benefits.

4.3 Placebo Test

Next, I perform a robustness test to alleviate the concern that my empirical specification mechanically captures a spurious effect. Specifically, I conduct a placebo analysis using charities in states without an audit mandate. First, I assign a placebo revenue threshold and presume that

charities that exceed this placebo threshold are required to obtain a financial audit. I then re-perform the analyses from Tables 2 and 3.[23] I replicate the procedure 500 times, randomly assigning a placebo threshold that falls between $300,000 and $3,000,000 (i.e., the range of the true threshold) in each iteration. In more than 99% of instances, I cannot reject the null hypothesis of no effect on governance mechanisms and private-benefit extraction. Figure 4 is a graphical representation of the RD design analog to Figures 2 and 3, but for one iteration of the placebo test where the placebo threshold drawn is $850,000. The figure shows essentially no discontinuity in any of the outcomes around the placebo audit threshold.

I tabulate the results for this iteration of the placebo test in Table 4. The odd-numbered columns report the estimates for the first stage. The coefficients are insignificant and close to zero, suggesting the placebo threshold has no effect on charities' propensity to be audited. The even-numbered columns report the second-stage estimates, which are all statistically indistinguishable from zero. In other words, I cannot reject the null that the placebo threshold has no effect on governance and managers' private benefits.[24] Overall, this placebo test alleviates the concern that my empirical specification mechanically generates spurious results.

4.4 Additional Robustness Tests

In addition, I highlight that the specifications I later introduce in sections 5.2 and 5.3 serve as supplemental robustness tests. Specifically, to operationalize cross-sectional analyses, I use a parsimonious reduced-form specification and a difference-in-differences design, and find that my main results are robust to using either of these alternative specifications.

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[23] The fact that certain states have no audit mandates renders this robustness test feasible. I could not perform this test in states with an audit mandate because the "true" thresholds would fall within the estimation bandwidths of the placebo thresholds. This would impair my ability to estimate the relation between the running variable and governance practices.

[24] It is not a concern that the second-stage estimates have large magnitudes, despite being statistically insignificant. Indeed, in such a two-stage model, an insignificant first stage mechanically inflates the second stage, which no longer captures a local average treatment effect (Angist and Pischke 2009, 2015).

5. Mechanism

Having explored the impact of financial audits on governance practices and agency costs, I now study the mechanism behind the observed effects. The channel I propose is that auditors evaluate their clients' governance practices as part of their internal-control assessment and that they recommend the implementation of specific governance mechanisms to the board of directors, who may lack insider access. The board then pressures management to implement the suggested governance measures.

5.1 Review Engagements to Factor Out the Role of the Internal-Control Assessment

I begin by evaluating whether financial reviews deliver similar effects on governance and private benefits as audits. Review engagements are a useful setting because they do not involve internal-control testing (AICPA 2022a SSAE 22.07), but they still offer assurance over financial reporting (although to a lesser degree than audits). Thus, finding no results for review engagements would corroborate that internal-control assessments are a key part of the mechanism driving the results in Tables 2 and 3.

Eleven of the 23 states with audit mandates also have mandates for financial reviews. A lower threshold prescribes which charities have to obtain a review. For example, in Georgia, charities with revenue above $500,000 ($1,000,000) must undergo a financial statement review (audit). I therefore re-perform the analyses in Tables 2 and 3 using review engagements instead of audits.

On the Form 990, the yes-or-no question I use to proxy for review engagements reads, "Were the organization's financial statements compiled or reviewed by an independent accountant?"[25] I acknowledge that this question introduces measurement error because I might

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[25] In a compilation engagement, the auditor does not deliver an opinion on the financial statements.

code charities as obtaining a review even if they only obtain a compilation.

In Table 5, I report the estimates of running Eq. (1) using review engagements. In columns 1, 3, 5, and 9, the estimates of the first stage are positive and strongly significant, which supports the relevance assumption for the three governance mechanisms as well as nepotism. The magnitudes are smaller than in Tables 2 and 3, which likely stems from the measurement error in proxying for charities that obtain reviews (see paragraph above). In the even-numbered columns, I report the second stage, which shows that none of the estimates are statistically significant, consistent with the prediction that financial reviews do not cause charities to implement governance mechanisms or reduce nepotism.[26] However, in column 7, the estimate of the first stage for the *CEO Pay Ratio* specification is not significant, suggesting that the relevance assumption is not met. One explanation for this result is that, because the thresholds for financial reviews are lower, many charities near the threshold are exempt from reporting CEO compensation, making the sample too sparse to conduct an RD analysis. Thus, for the *CEO Pay Ratio* specification, I cannot rule out that a lack of power (as opposed to a lack of economic effect) explains why review engagements do not have a statistically significant effect. Collectively, Table 5 documents no association between financial reviews and my outcomes of interest, consistent with the internal-control assessment (which is part of a financial audit, but not a review) being a key part of the mechanism by which audits affect governance practices and private-benefit extraction.

5.2 The Role of the Board of Directors

Next, I evaluate the role the board of directors plays on the effect of audits on governance

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[26] One might be concerned that, although the second-stage estimates are statistically insignificant, they are not close to zero. However, this is also likely driven by the measurement error in proxying for reviews, as a smaller first stage can mechanically inflate the magnitude of the second stage (Angrist and Pischke 2009, 2015).

practices. I predict that, to the extent management has voting power and control over the board, charities are less likely to heed (or receive) recommendations from the auditors about adopting governance measures that impair management's ability to extract private benefits.

In Table 6, I test whether the effects in Tables 2 and 3 are more pronounced for charities with an independent board of directors. As a preliminary step, I confirm that financial audits do not directly affect the board's independence, which means that I can use the board's independence as a cross-sectional variable. In column 1, I estimate Eq. (1) using *Board Independence* as the dependent variable. Comfortingly, the coefficient estimate is close to zero and not statistically significant.[27] This result is unsurprising because audit guidance instructs auditors to report to "those charged with governance," which implies that auditors can treat the governing body as given and are not responsible for the board's independence.

Next, I separately estimate Eq. (1) for charities with and without an independent board of directors for each of the outcomes from the tests in Tables 2 and 3. I report the second stage in columns 2 to 11 of Table 6. I consistently find that the effect of audits is more pronounced for charities with an independent board of directors. The only exception is *Nepotism,* where both coefficients are statistically insignificant (potentially due to the loss in statistical power from splitting the sample).

While the specification in Table 6 has the advantage of being methodologically consistent with the main analysis, it does not offer a formal statistical test. One reason for this is that the Calonico et al. (2014) method may select different parameters for each sample split. Therefore, I propose an alternative specification that allows me to perform such a statistical test. Specifically,

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[27] I find a similar "null result" when I replace the continuous *Board Independence* measure with a binary indicator equal to 1 if all voting board members are independent (untabulated).

I use a "reduced-form" specification, which regresses the dependent variables directly on *Audit Required* using OLS (Angrist and Pischke 2009, Breuer et al. 2018):

$$Y_{it} = \beta_0 + \beta_1 Audit\ Required_{it} + \beta_2 f(Distance\ to\ threhosld_{it}) + \beta_3 X + \varepsilon_{it} \quad \text{Eq. (2)}$$

I first use this specification to replicate the results in Tables 2 and 3. I then interact *Audit Required* with *Board Independence* to test whether the effects of financial audits are more pronounced for charities with an independent board. In contrast to the Calonico et al. method, this approach does not automatically select the RD parameters. To keep the model parsimonious, I select the following parameters: a bandwidth of 1.5, a linear control for the distance to the threshold, and the same covariates as in Eq. (1).

Table A2, Panel A, columns 1-5 replicate the main findings from Tables 2 and 3. Note that the reduced-form specification produces coefficient estimates that are smaller, but proportional to the causal effect of interest (Angrist and Pischke 2009). Column 6 documents no significant effect on *Board Independence* (which is similar to the results in Table 6, column 1), suggesting that *Board Independence* can be used in cross-sectional tests.

In Panel B of Table A2, the interaction term is statistically significant for *Whistleblower Policy*, *CEO Pay Approval*, and *Nepotism*, suggesting that the effect of audits is more pronounced for charities with an independent board of directors. However, the coefficient is not significant for the conflict-of-interest policies and for the CEO-to-employee pay ratio. For these two outcomes, the results of the cross-sectional test in Table 6 should therefore be interpreted with caution. Collectively, the findings in this section suggest that financial audits have a greater impact on governance when managers have less control over the board of directors.

5.3 The Role of the Audit Committee

To further shed light on the mechanism, I evaluate whether the effect of audits on governance depends on who selects and retains the auditor. I predict that when the decision lies

with management (as opposed to an audit committee), auditors are less likely to propose governance measures (which would curtail management's ability to extract private benefits) for fear of losing their recurring contract.

To test this prediction, I cannot rely on my RD design because organizations can only have an audit committee if they also obtain an audit. In other words, I have no way of cross-sectionally separating unaudited charities based on whether they would have an audit committee if they were audited. Thus, I use a difference-in-differences (DiD) design exploiting charities that grow to exceed the threshold over time. More specifically, my treatment group is comprised of charities that go from being unaudited to audited as they exceed the threshold. I create a control group from charities that remain unaudited and under the threshold throughout the sample period.

I begin by estimating the following DiD model to replicate the main results:

$$Y_{it} = \beta_0 + \beta_1 Audit_{it} + \beta_2 f(Distance\ to\ threhosld_{it}) + \beta_3 X + Charity\ FE + Year\ FE + \varepsilon_{it} \quad \text{Eq. (3)}$$

The specification includes a linear control for the distance to the threshold, the covariates from Eq. (1), and charity and year fixed effects. Unlike my RD design that relies on cross-sectional variation and thus captures long-run effects, my DiD design is suitable to capture short-run effects (Breuer 2021). For this reason, I focus on the governance mechanisms from Table 2, which are likely to manifest shortly after a charity obtains an audit for the first time. On the other hand, outcomes such as CEO compensation and nepotism manifest in the long run because they depend on employment contracts, which are negotiated infrequently.[28]

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[28] I caveat that my DiD is not as effective as my RD design in addressing endogeneity. For example, one concern with the DiD is that unobservable growth opportunities may prompt organizations to simultaneously obtain an audit and implement governance mechanisms in their attempt to raise external funding. As pointed out by Lee and Lemieux (2010) and Gow et al. (2016), RD designs "require seemingly mild assumptions compared to those needed for other nonexperimental approaches […] and causal inferences from RD designs are potentially more credible than those from typical 'natural experiment' strategies."

In the second step, I evaluate the differential effect of audit committees by re-estimating Eq. (3), while replacing *Audit* with *Audit with Audit Committee* and *Audit Without Audit Committee*. I acknowledge that the choice to have an audit committee is endogenous. For example, charities that choose to have an audit committee might also be more receptive to auditor recommendations, implying that, even without the audit committee, they might be more likely to change their governance in response to the audit.

In Table 7, column 1, I estimate Eq. (3) for *Conflict Interest Policy*. I find a positive and strongly significant relation, confirming the result in Table 2 is robust to the DiD specification. In support of the parallel assumption, I report Figure A1A. Comfortingly, I find parallel patterns during the pre-treatment period in *Conflict Interest Policy* for the control and treatment groups. In column 2 of Table 7, I break down *Audit* depending on whether the charity has an audit committee. I find that charities that have an audit committee along with the audit are 3 percentage points more likely to implement a conflict-of-interest policy than the charities that obtain an audit without a committee (i.e., $0.077 - 0.043 = 0.034$). A statistical test for the difference in coefficients rejects the null that *Audit with Audit Committee* and *Audit without Audit Committee* are equal (p-value = 0.012).

In columns 3-6 and Figures A1B and A1C, I estimate the specifications for *Whistleblower Policy* and *CEO Pay Approval,* and continue to find that the effect of audits is stronger for charities with an audit committee. In fact, for *CEO Pay Approval, Audit without Audit Committee* is not statistically significant, suggesting that audits only lead to the formal approval of CEO compensation when the charity has an audit committee. Taken together, these results suggest that an audit committee significantly increases the likelihood that charities adopt governance mechanisms after a financial audit.

6. Conclusion

This paper examines whether financial statement audits prompt organizations to change their governance practices, which can lead to a reduction in managers' ability to extract private benefits. Using a regression-discontinuity design, I find that obtaining an audit increases the probability that organizations have conflict-of-interest policies, whistleblower policies, and formal approval of CEO compensation by a committee. I also provide evidence of a reduction in CEO-to-employee pay ratio and nepotism.

I acknowledge that results in the nonprofit setting may not apply to all sectors of the economy. While I believe the findings can be insightful for types of organizations like local governments and private firms with noncontrolling investors, they may not be as relevant for public firms. Also, I estimate *local* average treatment effects around audit exemption thresholds, none of which exceed $3,000,000. Audits may affect larger organizations differently.

One important consideration is that, while governance mechanisms may reduce CEO compensation and nepotism, they may not necessarily benefit an organization. For example, allowing managers to hire a family member may be a cost-effective strategy to retain them. Future research can explore how directors account for such factors when deciding whether to implement governance mechanisms.

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Appendix A: State-Level Financial Statement Audit Regulation

Appendix A shows the states that have a financial audit mandate, along with their size-based exemption thresholds, as of the year 2015 (i.e., the end of my sample period).

State	Audit Regulation	Exemption Threshold (in thousands of $)	Specific Measure for Revenue Threshold	State	Audit Regulation	Exemption Threshold (in thousands of $)	Specific Measure for Revenue Threshold
Alabama	N			Montana	N		
Alaska	N			Nebraska	N		
Arizona	N			Nevada	N		
Arkansas	Y	500	Total Revenue	New Hampshire	Y	1,000	Total Revenue
California	Y	2,000	Total Revenue	New Jersey	Y	500	Total Revenue
Colorado	N			New Mexico	Y	500	Total Revenue
Connecticut	Y	500	Total Revenue	New York	Y	500	Total Revenue
District of Columbia	N			North Carolina	N		
Delaware	N			North Dakota	N		
Florida	Y	1,000	Revenue from Contributions	Ohio	N		
Georgia	Y	1,000	Total Revenue	Oklahoma	N		
Hawaii	Y	500	Total Revenue	Oregon	N		
Idaho	N			Pennsylvania	Y	300	Revenue from Contributions
Illinois	Y	300	Revenue from Contributions	Rhode Island	Y	500	Total Revenue
Indiana	N			South Carolina	N		
Iowa	N			South Dakota	N		
Kansas	Y	500	Revenue from Contributions	Tennessee	Y	500	Total Revenue
Kentucky	N			Texas	N		
Louisiana	N			Utah	N		
Maine	N			Vermont	N		
Maryland	Y	500	Revenue from Contributions	Virginia	N		
Massachusetts	Y	500	Total Revenue	Washington	Y	3,000	Total Revenue
Michigan	Y	525	Revenue from Contributions	West Virginia	Y	500	Revenue from Contributions
Minnesota	Y	750	Total Revenue	Wisconsin	Y	400	Revenue from Contributions
Mississippi	Y	500	Revenue from Contributions	Wyoming	N		
Missouri	N						

Appendix B: Variable Definitions

Variable	Definition
Conflict Interest Policy	A binary variable equal to 1 if the organization has a conflict-of-interest policy.
Whistleblower Policy	A binary variable equal to 1 if the organization has a whistleblower policy.
CEO Pay Approval	A binary variable equal to 1 if a committee formally approves the compensation of top management.
CEO Pay Ratio	The ratio of the CEO's compensation over the compensation of the average employee, in deciles.
Nepotism	A binary variable equal to 1 if distinct key employees, officers, and board members share the same last name.
Audit	A binary variable equal to 1 if the organization obtains an audit.
Audit Required	A binary variable equal to 1 if the organization's revenue exceeds the audit exemption threshold.
Revenue	The organization's total revenue in a given year.
Assets	The organization's total assets at the end of a given year.
Age	The difference between the current year and the year in which the organization was legally created.
Review	A binary variable equal to 1 if the organization obtains a financial review.
Review Required	A binary variable equal to 1 if the organization's revenue exceeds the review exemption threshold.
Board Independence	The number of independent voting directors divided by the total number of voting directors on the board.
Audit with Audit Committee	A binary variable equal to 1 if the organization obtains an audit and has an audit committee.
Audit without Audit Committee	A binary variable equal to 1 if the organization obtains an audit, but has no audit committee.

Figure 1: Distribution of Observations around the Audit Threshold

This figure shows the distribution of observations around the audit-exemption threshold. State-specific thresholds are centered at zero. Figure 1A is a histogram of the density of observations. Figure 1B is a density discontinuity plot produced using the Cattaneo et al. (2020) method, along with the related statistical test. The (log) distance to the threshold is the (log) difference between the revenue and the threshold.

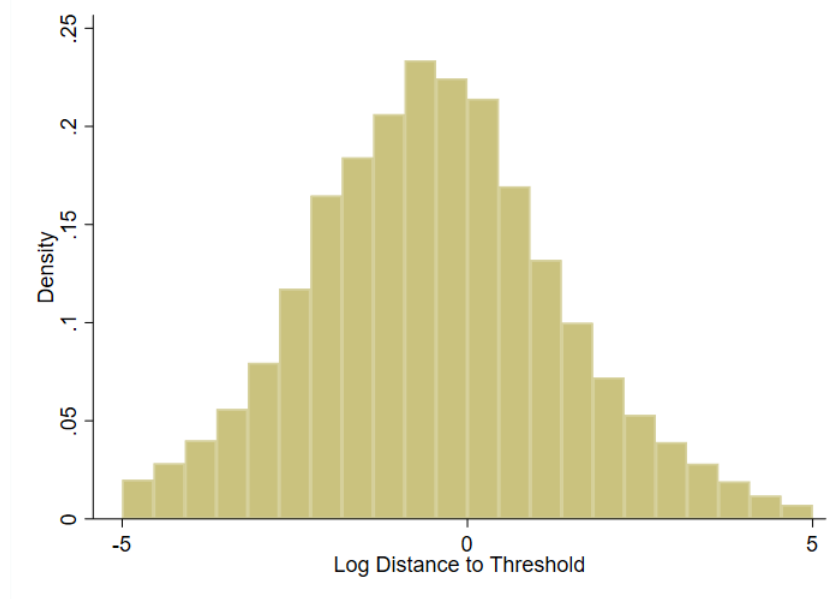Figure 1A: Histogram of Density of Observations around Audit Threshold

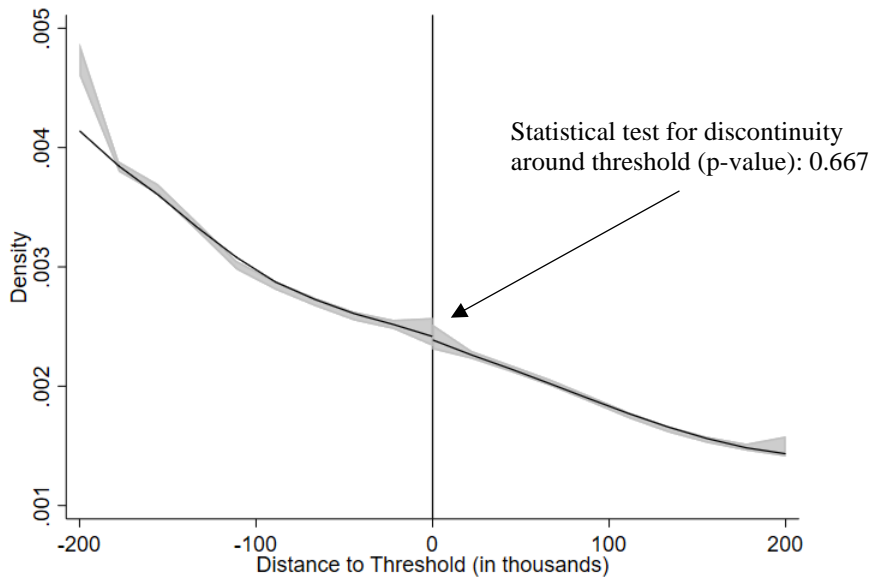Figure 1B: Density Discontinuity Plot and Statistical Test

Statistical test for discontinuity around threshold (p-value): 0.667

Figure 2: The Effect of Financial Audits on Governance Mechanisms
This figure presents binned scatter plots and fitted lines of the likelihood of implementing governance practices on the distance to the threshold. *Conflict Interest Policy* is a binary variable equal to 1 if the organization has a conflict-of-interest policy. *Whistleblower Policy* is a binary variable equal to 1 if the organization has a whistleblower policy. *CEO Pay Approval* is a binary variable equal to 1 if a committee approves the compensation of top management. The distance to the threshold is computed as the log difference between the revenue and the threshold. See Appendix B for variables definition.
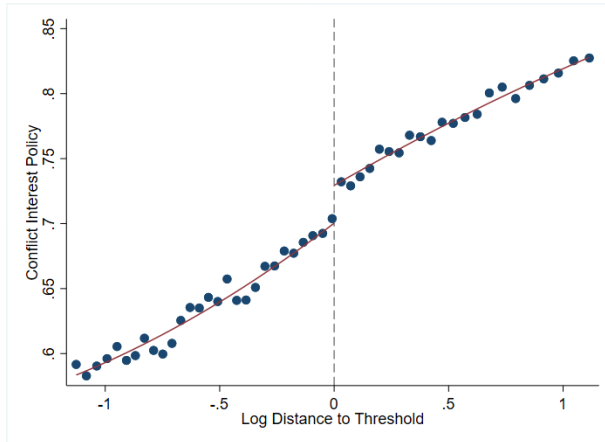
Figure 2A: Conflict of Interest Policy
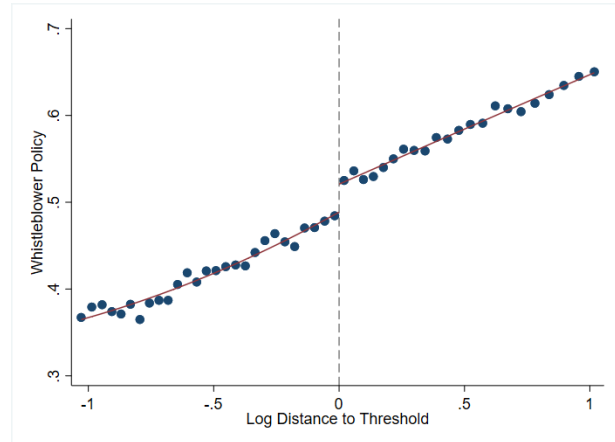
Figure 2B: Whistleblower Policy

Figure 2C: Approval of CEO's
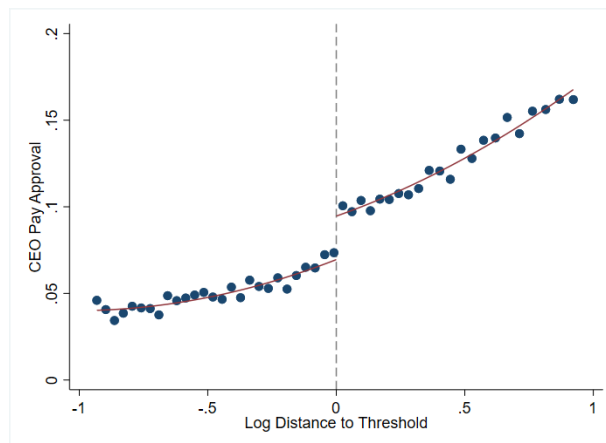Compensation by Committee

Figure 3: The Effect of Financial Audits on Managers' Private Benefits

This figure presents binned scatter plots and fitted lines of managers' private benefits on the distance to the threshold. *CEO Pay Ratio* is the ratio of CEO compensation over the compensation of the average employee in deciles. *Nepotism* is a binary variable equal to 1 if distinct key employees, officers, and board members share the same last name. The distance to the threshold is computed as the log difference between the revenue and the threshold. See Appendix B for variables definition.

Figure 3A: Ratio of CEO's Compensation over
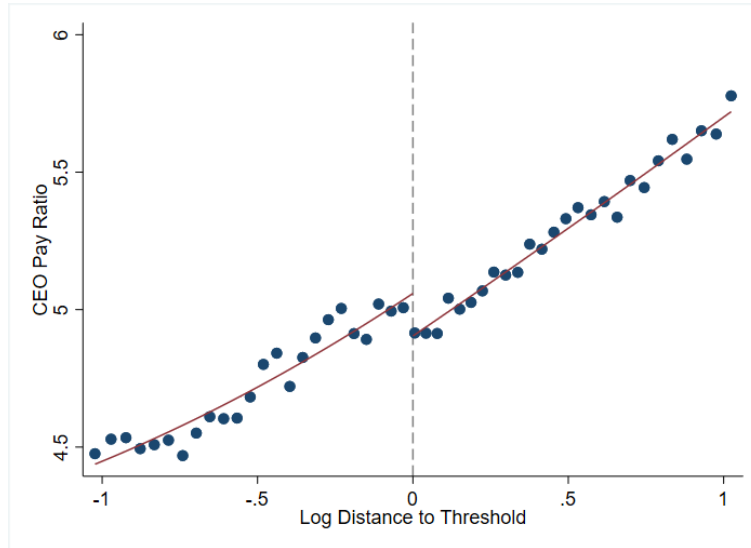Compensation of Average Employee (in deciles)
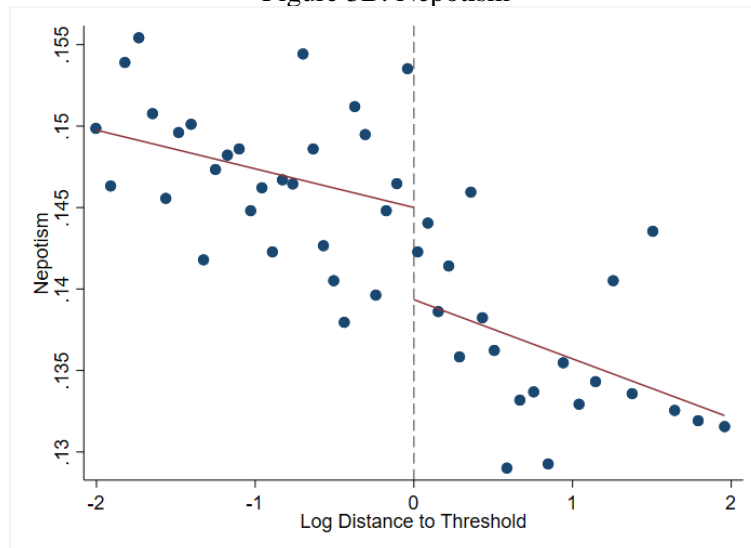

Figure 3B: Nepotism

Figure 4: Placebo Test

This figure presents binned scatter plots and fitted lines for one iteration of the placebo test that uses states without an audit mandate and a placebo threshold drawn from a random distribution. In this iteration, the placebo threshold is $850,000. The distance to the placebo threshold is computed as the log difference between the revenue and the placebo threshold. See Appendix B for variables definition.
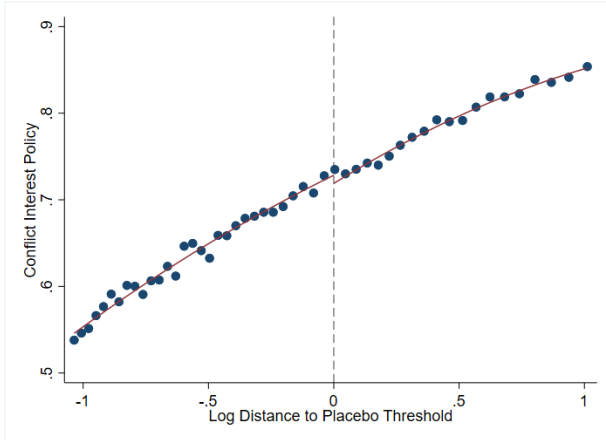
Figure 4A: Conflict of Interest Policy
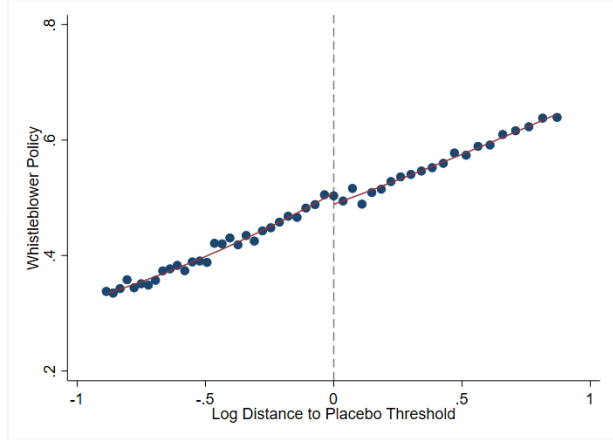
Figure 4B: Whistleblower Policy

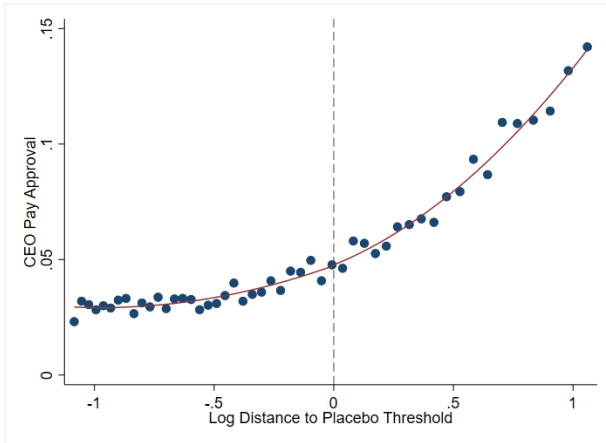Figure 4C: Approval of CEO Compensation by Committee

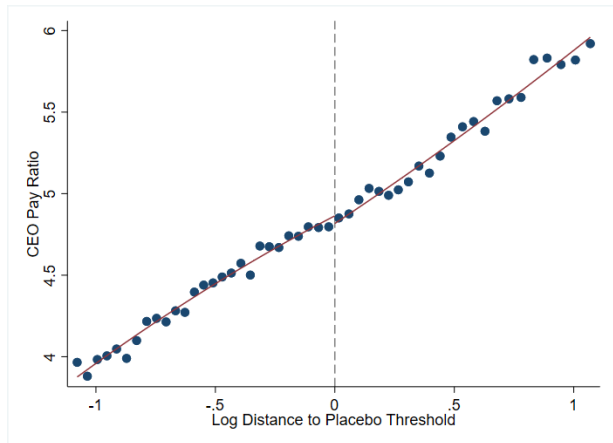Figure 4D: Ratio of CEO Compensation over Compensation of Average Employees (in deciles)

Figure 4E: Nepotism
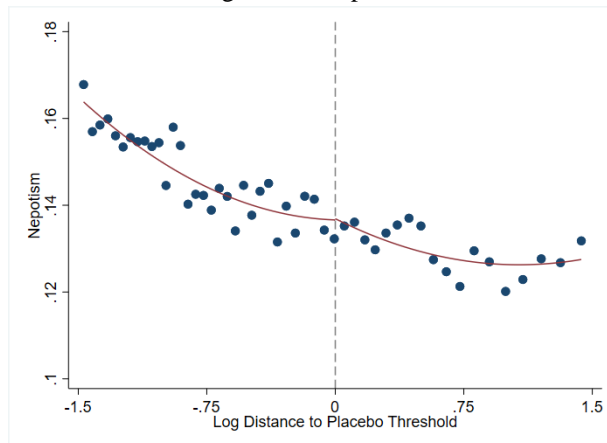
Table 1: Summary Statistics

This table reports the summary statistics for the variables used in Tables 2-7. The sample is based on Form 990 data from Guidestar for tax years 2008 to 2015. See Appendix B for variables definitions.

Variable	Mean	SD	P25	Median	P75	N
Conflict Interest Policy	0.662	0.473	0	1	1	712,969
Whistleblower Policy	0.471	0.499	0	0	1	712,969
CEO Pay Approval	0.121	0.326	0	0	0	712,969
CEO Pay Ratio (in deciles)	5.365	2.866	3	5	8	338,686
Nepotism	0.142	0.349	0	0	0	587,603
Audit	0.550	0.498	0	1	1	712,969
Audit Required	0.352	0.477	0	0	1	712,969
Revenue (in thousands)	3,893	35,034	214	519	1,652	712,969
Assets (in thousands)	7,818	110,174	186	748	2,819	712,969
Age	26.58	24.32	10	21	35	712,969
Review Required	0.285	0.451	0	0	1	712,969
Review	0.132	0.339	0	0	0	712,969
Independent Board	0.697	0.460	0	1	1	712,969
Audit with Audit Committee	0.457	0.498	0	0	1	712,969
Audit without Audit Committee	0.093	0.291	0	0	0	712,969

Table 2: The Effect of Financial Audits on Governance Mechanisms

This table presents the regression discontinuity estimates of Eq. (1) based on the Calonico et al. (2014) method. The method uses a data-driven approach to determine the bandwidth and the functional form of the running variable. For each outcome, the first- and second-stage estimates are reported. *Audit Required* is a binary variable equal to 1 if the organization's revenue exceeds the audit exemption threshold. *Audit* is a binary variable equal to 1 if the organization obtains an audit. *Conflict Interest Policy* is a binary variable equal to 1 if the organization has a conflict-of-interest policy. *Whistleblower Policy* is a binary variable equal to 1 if the organization has a whistleblower policy. *CEO Pay Approval* is a binary variable equal to 1 if a committee approves the compensation of top management. The distance to the threshold is computed as the log difference between the revenue and the threshold. The covariates are *LnAssets* and *LnAge*. The sample is based on Form 990 data from Guidestar for tax years 2008 to 2015. Robust standard errors clustered by charity are reported in parentheses. ***, **, and * indicate statistical significance at the 1 %, 5%, and 10% levels (two-tailed), respectively. See Appendix B for variables definition.

	Conflict of Interest Policy		Whistleblower Policy		CEO Pay Approval	
	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage
	Audit	*Conflict Interest Policy*	*Audit*	*Whistleblower Policy*	*Audit*	*CEO Pay Approval*
	(1)	(2)	(3)	(4)	(5)	(6)
Audit Required	0.127***		0.127***		0.128***	
	(0.006)		(0.007)		(0.006)	
Audit (instrumented)		0.204***		0.230***		0.190***
		(0.048)		(0.056)		(0.030)
f(distance to threshold)	Y	Y	Y	Y	Y	Y
Covariates	Y	Y	Y	Y	Y	Y
Number of observations	712,969	712,969	712,969	712,969	712,969	712,969
Effective observations	160,116	160,116	138,963	138,963	146,433	146,433

Table 3: The Effect of Financial Audits on Managers' Private Benefits
This table presents the regression discontinuity estimates of Eq. (1) based on the Calonico et al. (2014) method. The method uses a data-driven approach to determine the bandwidth and the functional form of the running variable. For each outcome, the first- and second-stage estimates are reported. *Audit Required* is a binary variable equal to 1 if the organization's revenue exceeds the audit exemption threshold. *Audit* is a binary variable equal to 1 if the organization obtains an audit. *CEO Pay Ratio* is the ratio of the CEO's compensation over the compensation of the average employee, in deciles. *Nepotism* is a binary variable equal to 1 if distinct key employees, officers, and board members share the same last name. The distance to the threshold is computed as the log difference between the revenue and the threshold. The covariates are *LnAssets* and *LnAge*. The sample is based on Form 990 data from Guidestar for tax years 2008 to 2015. Robust standard errors clustered by charity are reported in parentheses. ***, **, and * indicate statistical significance at the 1 %, 5%, and 10% levels (two-tailed), respectively. See Appendix B for variables definition.

	CEO Pay Ratio		Nepotism	
	1st Stage *Audit* (1)	2nd Stage *CEO Pay Ratio* (2)	1st Stage *Audit* (3)	2nd Stage *Nepotism* (4)
Audit Required	0.120***		0.135***	
	(0.008)		(0.007)	
Audit (instrumented)		-1.437***		-0.0643*
		(0.421)		(0.038)
f(distance to threshold)	Y	Y	Y	Y
Covariates	Y	Y	Y	Y
Number of observations	338,686	338,686	587,603	587,603
Effective observations	85,021	85,021	135,743	135,743

Table 4: Placebo Test

This table reports the regression discontinuity estimates for one iteration of the placebo test in which Eq. (1) is estimated using states without an audit mandate and a placebo threshold is drawn from a random distribution. In this iteration, the placebo threshold is $850,000. The estimates are based on the Calonico et al. (2014) method, which uses a data-driven approach to determine the bandwidth and the functional form of the running variable. For each outcome, the first- and second-stage estimates are reported. *Audit Required* is a binary variable equal to 1 if the organization's revenue exceeds the audit exemption threshold. *Audit* is a binary variable equal to 1 if the organization obtains an audit. The distance to the threshold is computed as the log difference between the revenue and the threshold. The covariates are *LnAssets* and *LnAge*. The sample is based on Form 990 data from Guidestar for tax years 2008 to 2015. Robust standard errors clustered by charity are reported in parentheses. ***, **, and * indicate statistical significance at the 1 %, 5%, and 10% levels (two-tailed), respectively. See Appendix B for variables definition.

	Conflict of interest policy		Whistleblower policy		CEO Pay Approval		CEO Pay Ratio		Nepotism	
	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage
		Conflict		*Whistleblower*		*CEO Pay*		*CEO Pay*		
	Audit	*Interest Policy*	*Audit*	*Policy*	*Audit*	*Approval*	*Audit*	*Ratio*	*Audit*	*Nepotism*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Audit Required	0.011		0.010		0.012		0.011		0.006	
	(0.009)		(0.009)		(0.009)		(0.012)		(0.010)	
Audit (instrumented)		-0.917		-1.550		0.147		-0.975		-0.502
		(0.833)		(1.137)		(0.284)		(4.347)		(0.744)
f(distance to threshold)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of observations	412,395	412,395	412,395	412,395	412,395	412,395	199,272	199,272	334,843	334,843
Effective observations	118,744	118,744	115,854	115,854	99,380	99,380	81,372	81,372	95,398	95,398

Table 5: Review Engagements to Factor Out the Role of the Internal-Control Assessment

This table presents the regression discontinuity estimates of Eq. (1) where *Audit Required* and *Audit* are replaced by *Review Required* and *Review*, respectively. The estimates are based on the Calonico et al. (2014) method, which uses a data-driven approach to determine the bandwidth and the functional form of the running variable. For each outcome, the first- and second-stage estimates are reported. *Review Required* is a binary variable equal to 1 if the organization's revenue exceeds the review exemption threshold. *Review* is a binary variable equal to 1 if the organization obtains a financial review. *Conflict Interest Policy* is a binary variable equal to 1 if the organization has a conflict-of-interest policy. *Whistleblower Policy* is a binary variable equal to 1 if the organization has a whistleblower policy. *CEO Pay Approval* is a binary variable equal to 1 if a committee approves the compensation of top management. The distance to the threshold is computed as the log difference between the revenue and the threshold. The covariates are *LnAssets* and *LnAge*. The sample is based on Form 990 data from Guidestar for tax years 2008 to 2015. Robust standard errors clustered by charity are reported in parentheses. ***, **, and * indicate statistical significance at the 1 %, 5%, and 10% levels (two-tailed), respectively. See Appendix B for variables definition.

	Conflict of interest policy		Whistleblower policy		CEO Pay Approval		CEO Pay Ratio		Nepotism	
	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage
	Review	*Conflict Interest Policy*	*Review*	*Whistleblower Policy*	*Review*	*CEO Pay Approval*	*Review*	*CEO Pay Ratio*	*Review*	*Nepotism*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Review Required	0.045***		0.032***		0.041***		0.021		0.046***	
	(0.009)		(0.010)		(0.009)		(0.015)		(0.010)	
Review (instrumented)		0.274		0.312		0.053		-3.137		-0.148
		(0.197)		(0.314)		(0.088)		(3.323)		(0.182)
f(distance to threshold)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of observations	241,773	241,773	241,773	241,773	241,773	241,773	85,257	85,257	195,944	195,944
Effective observations	99,591	99,591	61,543	61,543	80,795	80,795	32,441	32,441	66,970	66,970

Table 6: The Role of the Board of Directors

This table presents the regression discontinuity estimates of Eq. (1) based on the Calonico et al. (2014) method. The method uses a data-driven approach to determine the bandwidth and the functional form of the running variable. All columns report second-stage estimates. In column 1, the model is estimated using the full sample. In columns 2-11, the model is estimated separately for observations with and without an independent board of directors. *Audit Required* is a binary variable equal to 1 if the organization's revenue exceeds the audit exemption threshold. *Audit* is a binary variable equal to 1 if the organization obtains an audit. *Board Independence* is the number of independent voting directors divided by the total number of voting directors on the board. *Independent Board* is a binary variable equal to 1 if all voting board members are independent. The distance to the threshold is computed as the log difference between the revenue and the threshold. The covariates are *LnAssets* and *LnAge*. The sample is based on Form 990 data from Guidestar for tax years 2008 to 2015. Robust standard errors clustered by charity are reported in parentheses. ***, **, and * indicate statistical significance at the 1 %, 5% and 10% levels (two-tailed), respectively. See Appendix B for variables definition.

	Board Independence	Conflict Interest Policy		Whistleblower Policy		CEO Pay Approval		CEO Pay Ratio		Nepotism	
		Independent Board = 1	Independent Board = 0	Independent Board = 1	Independent Board = 0	Independent Board = 1	Independent Board = 0	Independent Board = 1	Independent Board = 0	Independent Board = 1	Independent Board = 0
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Audit (instrumented)	-0.016	0.215***	0.182***	0.239***	0.221***	0.204***	0.173***	-1.928***	-0.654	-0.050	-0.092
	(0.034)	(0.062)	(0.069)	(0.071)	(0.072)	(0.036)	(0.047)	(0.604)	(0.475)	(0.044)	(0.061)
f(distance to threshold)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of observations	712,969	496,695	216,274	496,695	216,274	496,695	216,274	232,241	106,445	416,710	170,893
Effective observations	123,665	99,944	62,757	93,294	60,229	97,149	55,050	55,269	39,273	101,803	47,336

Table 7: The Role of the Audit Committee
This table presents difference-in-differences estimates of Eq. (3) along with a version of the model that replaces *Audit* with *Audit with Audit Committee* and *Audit Without Audit Committee*. The treatment group consists of charities that go from unaudited to audited as they exceed the threshold. The control group consists of charities that are unaudited and fall under the threshold throughout the entire sample period. *Audit* is a binary variable equal to 1 if the organization obtains an audit. *Audit with Audit Committee* is a binary variable equal to 1 if the organization obtains an audit and has an audit committee. *Audit without Audit Committee* is a binary variable equal to 1 if the organization obtains an audit but has no audit committee. The distance to the threshold is computed as the log difference between the revenue and the threshold. The covariates are *LnAssets and LnAge*. The sample is based on Form 990 data from Guidestar for tax years 2008 to 2015. The F-test evaluates the statistical significance of the difference between *Audit with Audit Committee* and *Audit without Audit Committee*. Robust standard errors clustered by charity are reported in parentheses. ***, **, and * indicate statistical significance at the 1 %, 5%, and 10% levels (two-tailed), respectively. See Appendix B for variables definition.

	Conflict Interest Policy		Whistleblower Policy		CEO Pay Approval	
	(1)	(2)	(3)	(4)	(5)	(6)
Audit	0.067***		0.098***		0.034***	
	(0.012)		(0.012)		(0.008)	
Audit With Audit Committee		0.077***		0.109***		0.041***
		(0.012)		(0.013)		(0.010)
Audit Without Audit Committee		0.043***		0.070***		0.015
		(0.014)		(0.015)		(0.010)
Linear control for distance to threshold	Y	Y	Y	Y	Y	Y
Covariates	Y	Y	Y	Y	Y	Y
Charity fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Adjusted R-squared	0.918	0.918	0.906	0.906	0.671	0.671
Number of observations	141,911	141,911	141,911	141,911	141,911	141,911
F-test for difference of coefficients (p-value):		0.012		0.005		0.023

Figure A1: Difference-in-Differences Specification to Evaluate Effects of Financial Regulation on Governance Practices

This figure shows OLS regressions estimating the effect of financial audits on governance practices. I estimate the difference-in-differences model in Eq. (3), but replace the *Audit* indicator with separate time dummies that each mark a one-year period (except for event period t-1, which serves as the benchmark). The treatment group consists of charities that go from unaudited to audited as they exceed the threshold. The control group consists of charities that are unaudited and fall under the threshold throughout the entire sample period. The figure plots coefficient estimates for each year with 99% confidence intervals. See Appendix B for variables definition.

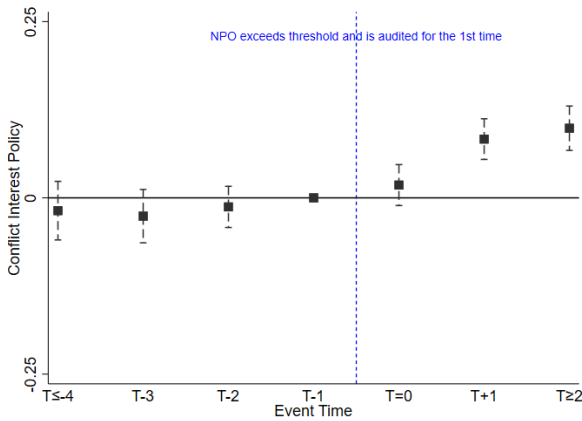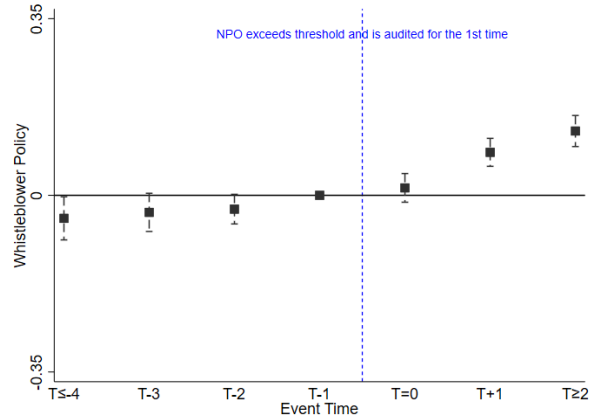Figure A1A: Conflict of Interest Policy

Figure A1B: Whistleblower Policy

Figure A1C: Approval of CEO's
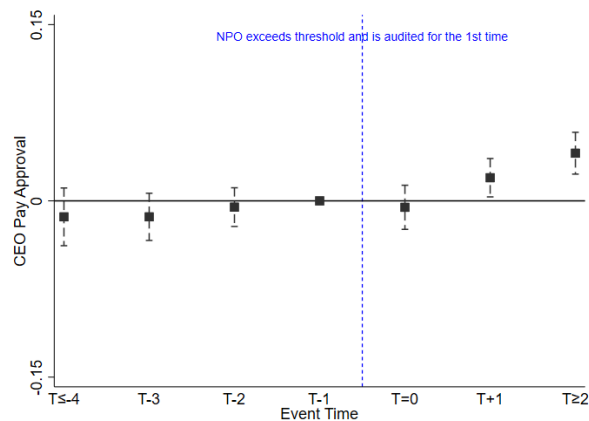Compensation by Board of Directors

Table A1: Robustness to Including State and Year Fixed Effects

This table presents the regression discontinuity estimates of Eq. (1) (including state and year fixed effects) based on the Calonico et al. (2014) method. The method uses a data-driven approach to determine the bandwidth and the functional form of the running variable. For each outcome, the first- and second-stage estimates are reported. *Audit Required* is a binary variable equal to 1 if the organization's revenue exceeds the audit exemption threshold. *Audit* is a binary variable equal to 1 if the organization obtains an audit. *Conflict Interest Policy* is a binary variable equal to 1 if the organization has a conflict-of-interest policy. *Whistleblower Policy* is a binary variable equal to 1 if the organization has a whistleblower policy. *CEO Pay Approval* is a binary variable equal to 1 if a committee approves the compensation of top management. *CEO Pay Ratio* is the ratio of the CEO's compensation over the compensation of the average employee, in deciles. *Nepotism* is a binary variable equal to 1 if distinct key employees, officers, and board members share the same last name. The distance to the threshold is computed as the log difference between the revenue and the threshold. The covariates are *LnAssets* and *LnAge*. The sample is based on Form 990 data from Guidestar for tax years 2008 to 2015. Robust standard errors clustered by charity are reported in parentheses. ***, **, and * indicate statistical significance at the 1 %, 5%, and 10% levels (two-tailed), respectively. See Appendix B for variables definition.

	Conflict of interest policy		Whistleblower policy		CEO Pay Approval		CEO Pay Ratio		Nepotism	
	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage	1st Stage	2nd Stage
	Audit	*Conflict Interest Policy*	*Audit*	*Whistleblower Policy*	*Audit*	*CEO Pay Approval*	*Audit*	*CEO Pay Ratio*	*Audit*	*Nepotism*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Audit Required	0.126***		0.129***		0.127***		0.123***		0.135***	
	(0.007)		(0.007)		(0.006)		(0.008)		(0.007)	
Audit (instrumented)		0.188***		0.215***		0.146***		-0.964**		-0.0824**
		(0.053)		(0.052)		(0.030)		(0.414)		(0.040)
f(distance to threshold)	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Number of observations	712,969	712,969	712,969	712,969	712,969	712,969	338,686	338,686	587,603	587,603
Effective observations	130,151	130,151	184,848	184,848	151,488	151,488	89,456	89,456	128,885	128,885

Table A2: Reduced-Form Specification to Evaluate the Role of the Board of Directors

This table presents OLS estimates of Eq. (2). Panel A reports reduced-form estimates for the dependent variables in Tables 2 and 3 as well as for *Board Independence*. Panel B reports estimates of the interaction between *Audit Required* and *Board Independence* for the dependent variables in Tables 2 and 3. *Audit Required* is a binary variable equal to 1 if the organization's revenue exceeds the audit exemption threshold. *Audit* is a binary variable equal to 1 if the organization obtains an audit. *Board Independence* is the number of independent voting directors divided by the total number of voting directors on the board. The covariates are *LnAssets* and *LnAge*. The bandwidth is 1.5 and a linear functional is used to control for the running variable, which is the log difference between the revenue and the threshold. The sample is based on Form 990 data from Guidestar for tax years 2008 to 2015. Robust standard errors clustered by charity are reported in parentheses. ***, **, and * indicate statistical significance at the 1 %, 5%, and 10% levels (two-tailed), respectively. See Appendix B for variables definition.

Panel A: Replication Using Reduced-Form Specification

	Conflict Interest Policy	Whistleblower Policy	CEO Pay Approval	CEO Pay Ratio	Nepotism	Board Independence
	(1)	(2)	(3)	(4)	(5)	(6)
Audit Required	0.035***	0.041***	0.023***	-0.151***	-0.006*	-0.002
	(0.004)	(0.005)	(0.003)	(0.034)	(0.003)	(0.003)
f(distance to threshold)	Y	Y	Y	Y	Y	Y
Covariates	Y	Y	Y	Y	Y	Y
Adjusted R-squared	0.067	0.092	0.075	0.114	0.001	0.030
Number of observations	372,321	372,321	372,321	197,807	310,080	372,321

Panel B: Cross-Sectional Test Based on Reduced-Form Specification

	Conflict Interest Policy	Whistleblower Policy	CEO Pay Approval	CEO Pay Ratio	Nepotism
	(1)	(2)	(3)	(4)	(5)
Audit Required × *Board Independence*	0.003	0.058***	0.035***	0.151	-0.029***
	(0.010)	(0.010)	(0.005)	(0.092)	(0.009)
Audit Required	0.033***	-0.009	-0.008	-0.287***	0.020**
	(0.010)	(0.009)	(0.005)	(0.090)	(0.009)
Board Independence	0.270***	0.190***	-0.016***	0.187***	-0.060***
	(0.006)	(0.005)	(0.002)	(0.058)	(0.005)
f(distance to threshold)	Y	Y	Y	Y	Y
Covariates	Y	Y	Y	Y	Y
Adjusted R-squared	0.095	0.106	0.075	0.114	0.004
Number of observations	372,321	372,321	372,321	197,807	310,080